established. The Nebula, being circumpolar both at Hobart Town and Sydney, would (irrespective of the very great differences of altitude under which it was probably observed) present itself to the observers in every possible variety of position, as to *up or down*, and would I believe *therefore* occasion some differences of estimation of form and relative intensity of the different parts. Further, the variety of instruments used, generally of very inferior power to that of Sir John Herschel, and the general want of precision in most of the observations recorded, diminish greatly their respective weight. Again, the declension of magnitude of η, from nearly the 1st magnitude to the 6th or 7th, would of itself cause the respective brightness of the different parts of the surrounding nebula to be very differently estimated.

Under all these circumstances and conditions of this very conflicting testimony, I incline to the opinion that *no proof has yet been given of any change in the Nebula at all.* And, finally, if the subject be not already exhausted, I would refer to another letter from Capt. Herschel, dated 7th May, 1871, and inserted in the *Monthly Notice*, for June last.

Ray Lodge, Maidenhead,
19th July, 1871.

Theoretical Considerations respecting the Corona : Part II.

By R. A. Proctor, B.A. (Cambridge).

Before entering upon the consideration of the remaining portion of my subject, I propose briefly to discuss some questions which have been raised since the former part was written. I refer in particular to the meteoric theory of the Corona,—that is, the theory that a large proportion of the light of the Corona is due to the existence of countless hosts of meteors in the Sun's neighbourhood. I have mentioned already my belief that I had somewhat exaggerated the share which must be assigned to meteoric systems in accounting for coronal phenomena.* But this admission must

* In a paper by Prof. C. A. Young, of America, entitled "Note on the Spectrum of the Corona" (one of a series of highly suggestive and valuable contributions to the theory of the subject), he says, "Although I am not able to admit with Mr. Proctor that the whole explanation of the Corona is involved in the presence of such meteoric particles, yet it cannot be doubted that they are very numerous; and any that may come within 250,000 miles of the solar surface must become incandescent, &c." I cannot remember having on any occasion asserted that the whole explanation of the Corona is involved in the existence of meteoric systems near the Sun. I could quote many passages implying that I hold the contrary opinion. I may have written at times about the meteoric explanation as founded on a *vera causa* without discussing other *veræ causæ;* but it is not, therefore, to be inferred that I have doubted the reality of these others. I would notice in particular that I have always believed in the atmospheric nature of a portion of the light seen around the Sun during totality—even at the middle of the totality. In a paper in the *Monthly Notices* for March 1870, I note that the illumination of our atmosphere by the light of the prominences and sierra should result in "a faint diffused light diminishing

not be understood as having any reference whatever to opinions which I have publicly expressed. On the contrary, all my *statements* respecting the competence of the meteoric theory to account for the phenomena of the Corona, have been so guarded that I find nothing in them to modify. In speaking of a change of opinion, I have referred only to that view of the subject which I had been disposed to entertain in my own mind, not to any opinions which I had definitely enunciated. But I am not concerned at present to indicate how far, or how little, my own opinions have changed. Objections have recently been raised which would tend, if admitted, to invalidate the meteoric theory altogether as a means of explaining any portion of the coronal phenomena. These objections must be dealt with here, because they touch the very basis of the theory I am now considering. I refer to those remarks in Sir W. Thomson's able address at the meeting of the British Association in Edinburgh, in which he referred to the opinions he had once entertained respecting the meteoric origin of the solar heat.

The meteoric theory of the solar heat supply cannot be regarded as demonstrated, or even (considered as the sole account of the solar heat) as demonstrable; nor am I here anxious to support it in any way. But it is important to notice that the meteoric theory advanced and eventually abandoned by Sir W. Thomson is not the theory to which recent astronomical discoveries have pointed; nor can the reasoning which Sir W. Thomson has advanced as demonstrative against his own theory be urged with equal force (if with any force at all) against those views as to meteoric matter in the Sun's neighbourhood, which could alone be now advocated by the student of meteoric astronomy. It must be remembered that Sir W. Thomson's theory related to meteors circling chiefly within the orbit of the Earth, and he was led to abandon it "because Leverrier's researches on the motion of the planet *Mercury*, though giving evidence of a sensible influence attributable to matter circulating as a great number of small planets within *Mercury's* orbit, showed that the amount of matter which could possibly be assumed to circulate at any considerable

towards the neighbourhood of the Moon," and "extending over the Moon's disk (since it would illuminate the air between the observer and the Moon's body)." I need hardly observe that the same obvious reasoning which showed me that the prominences and sierra must produce this kind of illumination, convinced me also that the real solar Corona (extending beyond the highest prominences) must be added to the causes of this atmospheric illumination; but in a paper written expressly to show that there *is* such a solar Corona, I was not free to assume in the opening paragraphs the very point I sought to prove. As surely as the visibility of the prominences and sierra implies the existence of an atmospheric halo due to their light, so surely the visibility of a real solar Corona implies the visibility of an atmospheric halo due to the light of that Corona. That both haloes are very faint compared with the real solar Corona follows from the reasoning given in the same paper (*Monthly Notices*, vol. xxx., p. 142), that is (as Prof. Young and Dr. Balfour Stewart have since severally shown) from the darkness of the Moon's disk during totality, and from the faintness of the ordinary glare round the Sun by comparison with the light of the solar disk.

distance from the Sun must be very small;" therefore, "if the meteoric influx taking place at present is enough to produce any appreciable portion of the heat radiated away, it must be supposed to be from matter circulating round the Sun within very short distances of his surface. The density of this meteoric cloud would have to be supposed so great that comets could scarcely have escaped, as comets actually have escaped, showing no discoverable effects of resistance, after passing his surface within a distance equal to one-eighth* of his radius." But recent discoveries respecting meteors point to a totally different solution of the difficulty here considered. We are neither bound to show that the greater part of the meteoric matter available for the purpose in question is at any time present within the Earth's orbit, nor that the matter which is at any time so placed (or rather at a less distance from the Sun than our Earth) is the same matter which is similarly circumstanced at another time. All that we know respecting meteor-systems teaches us to regard them as, for the most part, travelling in very eccentric orbits, only a small part of each orbit lying at a less distance than the Earth from the Sun, and therefore only a small portion of each system being at any time nearer to the Sun than the Earth is. And further, we have every reason for believing that only a very small proportion of the meteoric systems travel near to the plane of the Earth's orbit. Among the meteoric orbits there is every variety of inclination; and, therefore, we have to deal with matter which is not collected in or near the plane of the ecliptic, but with matter completely enveloping the Sun on all sides. It is not a disk or very flat spheroid, but a sphere of meteoric matter, that we are concerned with in considering those portions of such matter which lie nearer to the Sun than our Earth does. It needs but a slight acquaintance with the laws of planetary motion to see that neither the motion of the Earth (and the inappreciable change in the length of the year), nor the motion of *Mercury* (and the slowness of the change in the position of his perihelion), can afford such significant evidence respecting the quantity of meteoric matter existing within given distances of the Sun, as was formerly supposed.

In like manner, another objection which Sir W. Thomson has urged against his former views loses much of its force when considered with reference to accepted meteoric theories. "Spectrum analysis," says Sir W. Thomson, "gives proof finally conclusive against the hypothesis that the Sun's heat is supplied dynamically from year to year by the influx of meteors. Each meteor circulating round the Sun must fall in along a very gradual spiral path, and before reaching the Sun must have been for a long time exposed to an enormous heating effect from his radiation when very near, and must thus have been driven into vapour before

* It should be, I conceive, "one-*fifth*," the comet referred to being that of the year 1843.

actually falling upon the Sun. Thus, if Mayer's hypothesis is correct, friction between vortices of meteoric vapours and the Sun's atmosphere must be the immediate cause of solar heat; and the velocity with which these vapours circulate round the equatorial parts of the Sun must amount to 435 kilomètres per second. The spectrum test of velocity applied by Lockyer showed but a twentieth part of this amount as the greatest observed relative velocity between different vapours in the Sun's atmosphere."

Now if this objection is sound as against the meteoric theory of the solar-heat supply, it is sound also as against the very existence of meteoric systems close to the Sun, a conclusion which very few will be disposed to admit after what has recently been discovered respecting meteors and comets. But I apprehend that the objection is obviated by precisely the same reasoning which is valid against the former objection. Undoubtedly if meteoric matter came in vortically around the equatorial parts of the Sun and in the direction of planetary motion, we might possibly expect to find some spectroscopic evidence of the existence of their vaporous substance, moving as it would with a velocity exceeding more than 200 times that due to the Sun's rotation. Even in this case it would appear venturesome to assert that the want of such evidence was "proof finally conclusive against" the existence of the meteoric vortices. For the meteoric matter being brought to rest (relatively to the Sun) by friction, there must be all possible rates of motion between 240 miles per second and rest with respect to the Sun's globe. The bright lines due to the vaporous meteors would, therefore, be widened so as to cover the space between their normal position and the positions due to the maximum velocity of 240 miles per second. That thus widened, and proportionately faint, they would be discernible as bright lines on the bright background of the solar spectrum, may be gravely doubted; that they *must* be so discernible may be safely denied. But the actual circumstances are very different even from those here considered. All the evidence recently obtained respecting meteors tends to show that those which approach the Sun neither "travel around his equatorial parts," nor move on a direct course, nor with a velocity that can be definitely assigned. Our Earth encounters more than 100 meteor systems (according to the observations of Heis, Alexander Herschel, and others), and though none of these systems probably pass near the Sun, yet we can infer from them what must be the characteristics of the millions on millions of meteoric systems which (according to all reasonable probability) belong to the solar system. We must conclude, then, that the systems which pass near the Sun are inclined in all possible directions to the solar equator, have every possible degree of eccentricity (and, therefore, every degree of velocity between 240 and 379 miles per second), and travel both in direct and retrograde courses. That the spectroscope would afford any evidence of the existence of these multifarious forms and degrees of motion, existing in systems which probably

include every variety of elementary constitution, and further modified by the effects of frictional resistance, so that every velocity down to relative rest must be included among the meteoric movements, is utterly improbable, to say the least.

It will be understood that if the objections here considered were valid, they would affect the theory that the Sun expels meteoric matter from his interior, as fatally as the general theory that meteoric systems are circulating in countless myriads around the Sun's globe. For the Sun is but one among the unnumbered millions of suns which exist throughout space, and if our Sun expels meteoric matter it must be inferred that the stars act in like manner. And objections against the existence of meteoric systems around the Sun—which cannot possibly be Sun-expelled—would be objections against star-expelled meteors, and so inferentially against the expulsion of meteoric matter from the Sun. It is on this account that I have thought it necessary to consider the objections dealt with above.

If any of the meteors which reach our Earth have really been expelled either from our own Sun or from his fellow-suns, we might expect that their structure, as well microscopic as chemical, would exhibit some signs of the circumstances under which these bodies had their origin.

Now as respects the microscopic structure of meteors, although we have much interesting information, and though some facts are known which seem scarcely explicable save on the strange theory I am considering, yet it must be admitted that there is much that is perplexing. Since I wrote the first part of this paper, I have had the opportunity of inspecting a large number of Mr. Sorby's singularly beautiful specimens, with his own instruments, and with the advantage of his own unrivalled experience to explain those facts which otherwise would have had little meaning to me. He also kindly gave me copies of all his papers on the subject, and these I have carefully studied. Space will not permit me to discuss here the various facts on which Mr. Sorby's reasoning and his (hypothetical) conclusions have been based. It must suffice for me to state, that while there remain many sources of perplexity in every part of the subject, he still considers the general conclusions which he published in 1864 as the most probable, and that, in fact, no other seems available. How far this conclusion is in accordance with the theory we are upon, the reader shall judge. "The most remote condition of which we have positive evidence," he wrote in 1864, "was that of small detached melted globules, the formation of which cannot be explained in a satisfactory manner, except by supposing that their constituents were originally in the state of vapour as they exist in the Sun." He found evidence that the meteors had been in the state of vapour while under enormous pressure, and "in mountain masses." It certainly seems difficult to understand where and how the substance of meteors could have been in this state,

save within an orb as intensely heated and as vast as our Sun and his fellow-suns.

The evidence from the chemical structure of meteors is even more striking.

If we consider the circumstances under which the meteors are supposed (according to this theory) to be expelled from the Sun or stars, and remark the evidence we have respecting the existence of hydrogen in other suns than ours, we shall see the probability that some among the meteors which reach us would show signs of having been once surrounded by intensely hot hydrogen, existing at an inconceivably vast pressure. For iron, which is so frequently present in meteoric masses, if solidified under such conditions, would condense within its substance a considerable proportion of hydrogen. Now we have evidence on excellent authority that meteoric iron contains a larger amount of occluded hydrogen than malleable iron can be impregnated with. The late Professor Graham examined a piece of the Lenarto meteor,—constituted, according to Werle's analysis, of 90·883 parts of iron, 8·450 parts of nickel, 0·665 of cobalt, and 0·002 of copper. When a volume of 5·78 cubic centimètres of this iron was heated to redness, "gas came off rather freely; namely, in 35 minutes 5·38 cubic centimètres, in the next 100 minutes 9·52, and in the next 20 minutes 1·63 cubic centimètres," in all, in rather more than 2½ hours, no less than 16·53 centimètres, or about 3 times the volume of the iron itself. "The first portion of the gas collected had a slight odour," says Professor Graham,* "but much less than the natural gases occluded by ordinary iron. It did not contain a trace of carbonic acid." The second portion of the gas collected (consisting of 9·52 cubic centimètres) gave of hydrogen 85·68 parts per cent, the rest consisting of nitrogen and carbonic oxide. "The Lenarto iron appears, therefore, to yield 2·85 times its volume of gas," says Professor Graham, "of which 86 per cent nearly is hydrogen, the proportion of carbonic oxide being so low as 4½ per cent." But "the gas occluded by iron from a carbonaceous fire is very different, the prevailing gas then being carbonic oxide. For comparison a quantity of clean horseshoe nails was submitted to a similiar distillation." This iron gave 2·66 times its volume of gas; the first portion collected contained only 35 per cent of hydrogen, 50·3 per cent being carbonic oxide, 7·7 per cent carbonic acid, and 7 per cent nitrogen; the second portion gave no carbonic acid, but 58 per cent of carbonic oxide, and only 21 per cent of hydrogen.

On these results Professor Graham reasons as follows:—"It has been found difficult to impregnate malleable iron with more than an equal volume of hydrogen under the pressure of our atmosphere. Now the meteoric iron (this Lenarto iron is remark-

* Not having Graham's original paper by me, I quote these passages from extracts in Mr. Mattieu Williams' *Fuel of the Sun*, where the theory of the expulsion of meteors from the Sun is enunciated and supported—on grounds, however, not always strictly in accordance with dynamical principles.

ably pure and malleable) gave up about three times that amount without being fully exhausted. The inference is that *the meteorite had been extruded from a dense atmosphere of hydrogen gas*, for which we must look beyond the light cometary matter floating about within the limits of our solar system. . . . Hydrogen has been recognized in the spectrum analysis of the light of the fixed stars by Messrs. Huggins and Miller. The same gas constitutes, according to the wide researches of Father Secchi, the principal element of a numerous class of stars, of which α *Lyræ* is the type. *The iron of Lenarto iron has no doubt come from such an atmosphere, in which hydrogen greatly prevailed. This meteorite may be looked upon as holding imprisoned within it, and bearing to us, the hydrogen of the stars.*"

Other circumstances relating to the Corona itself seem to require some such theory as that we are dealing with for their elucidation.

The coronal spectrum, although not by any means identical with the spectrum of the terrestrial aurora, show yet such a resemblance to this spectrum as to indicate that the Corona is in part due to a perpetual solar aurora. Such at least is the theory to which many profound reasoners have been led by the study of the coronal spectrum. But a difficulty had existed in determining how electrical action could be excited where we see the light of the Corona.* The theory we have been dealing with would remove this difficulty, for the rush of the erupted matter, even through the rare medium existing round the Sun, would produce precisely the effect which the coronal theory requires. The fact that one of the lines of the coronal spectrum belongs to the spectrum of iron may be regarded as supplying subsidiary evidence of some weight.

I have already referred to the fact that under close telescopic scrutiny the Corona presents close by the Sun an appearance as though countless thousands of jets were issuing from the photosphere. But it may be asked whether any direct evidence of an outrush of matter has ever been obtained? It might well happen that no such evidence was available; for, as I have mentioned, the actual volume of the erupted matter must be supposed to bear but the minutest possible proportion to the volume of the prominences. The swift motion of the erupted matter would not tend,

* When Prof. Reynolds exhibited (at the last meeting of the British Association) the very beautiful electrical Corona by which he illustrates the auroral theory of the Corona, Prof. Tait remarked that this theory had been rejected by men of science. It is difficult to understand on what grounds this remark was founded. I cannot find that any man of science has expressed an opinion adverse to the auroral theory. Dr. Balfour Stewart, General Sabine, and others, have used arguments respecting the prominences (before the nature of these was known) which may now be fairly applied to the Corona, while Prof. Young of America, Prof. Reynolds, and others in England, and several continental physicists, have spoken favourably of the auroral theory as directly applicable to the phenomena of the Corona. The noteworthy point is not, however, that there is such good authority in favour of the theory, but that not one man of science has definitely expressed an opinion adverse to it.

perhaps, to add to the difficulty of detection, because the effects of that motion at the Sun's distance would scarcely be appreciable, even in powerful telescopes. But it would be difficult to distinguish the erupted matter by its appearance, and as its light would give a continuous spectrum (owing to the enormous compression of the issuing jets), it would be wholly impossible to detect the existence of this matter by spectroscopic analysis. It may be questioned whether the brilliant flakes seen by Mr. Gilman in the large prominences visible during the Eclipse of 1869 can be regarded as in any way related to the subject we are upon. These flakes "stood out," he says, "as if totally unconnected from the rest of the prominence." But their size, as described and pictured by him, forbids us to believe that they could have been masses of erupted matter; though it is by no means impossible that they may have been clusters of many such masses resulting from volleyed discharges.

A phenomenon observed by Dr. Zöllner seems less questionably related to our subject. Observing the Sun on June 27, 1869, he noticed that as soon as he brought the slit of the spectroscope close to a certain part of the Sun's limb, *where the prominences were particularly long and bright*, brilliant linear flashes passed through the whole length of the dull spectrum, over the limb of the Sun, about three or four minutes' distance from the latter, "These flashes," he says, "passed over the whole of the spectrum in the field of view, and became so intense at a certain point of the Sun's limb as to produce the impression of a series of electrical discharges rapidly succeeding one another, and passing through the whole spectrum in straight lines. Mr. Vogel, who afterwards, for a short time, took part in these observations, found the same phenomenon at a different portion of the Sun's limb, *where protuberances also appeared.*" Zöllner remarks that "the phenomenon can be explained by the hypothesis that small intensely incandescent bodies moving near the surface of the Sun emit rays of all degrees of refrangibility, and produce flashes of a thread-like spectrum as their image passes before the slit of the spectroscope."

To these considerations may be added some which are connected with the aspect of the solar photosphere. For instance, the researches of De La Rue, Stewart, and Loewy, seem to prove that "the faculæ of a spot have been uplifted from the very area occupied by the spot, and have fallen behind from being thrown up into a region where the velocity of rotation is greater." This, of course, would correspond with what the theory we are considering would suggest. "And it may be noticed," here I quote from a paper of my own in *Fraser's Magazine*, for April 1871, "that, regarding spots as phenomena of eruption, that is, as *beginning* with eruption, we can find a reason for their occurrence being associated, as Mr. De La Rue and his colleagues believe, with the relative proximity of the planets. For eruptions and earthquakes on our own Earth, stable as its substance un-

doubtedly is by comparison with the Sun's, have been observed to occur more frequently when the Moon is in perigee; and Sir John Herschel has explained the predominance of active volcano and earthquake regions along shore-lines as depending on the seemingly insignificant changes due to tidal action. How much more, therefore, might we expect that the solar equilibrium would be disturbed by planetary action, when all that has been revealed respecting the Sun tends to show that the mightiest conceivable forces are always at work beneath his photosphere, one or other needing only (it may well be) the minutest assistance from without to gain a temporary mastery over its rivals. And if, as recent observations tend to show, the mightiest of the planets sympathises with solar action,—if when the Sun is most disturbed the belts of *Jupiter* are also subject (as of late and in 1860) to strange phenomena of change,—how readily do we find an explanation of what would otherwise seem so mysterious, when we remember that, as *Jupiter* disturbs the mighty mass of the Sun, so the Sun would reciprocally disturb the mass of the largest of his attendant orbs."

Brighton, September, 1871.

Observations of Saturn, Mars, &c. By Rev. J. Spear.

I am sorry I have not been able to record any observations of importance, except perhaps the error in the calculation of the occultation of ζ *Tauri*. I send, however, the following, which may, of course, be used as the Society may think fit.

Place of observation at the following dates mentioned herein :

Latitude	$30^{\circ}\ 42'\ 4''$
Longitude	$5^{h}\ 11^{m}\ 42^{s}$ E.
Elevation	6603 feet.

June 13th, 1870. Observed occultation of *Saturn* by the Moon. Definition *excellent*. The Moon passed steadily over the planet without causing any change of form, or giving *any* indication of the planet's light passing through an atmospheric medium. The planet, when near the Moon's limb, assumed a "sickly green hue," according to a note I made at the time, and which I saw corroborated in the *Monthly Notice* afterwards.

Nov. 9th, 6.30 A.M. Observed *Mars*. Phase gibbous. Snow and ice on the north pole, *intensely bright and glistening*. P. 240. Aperture $4\frac{1}{4}$ inches. *Jupiter*, at the same time, appeared covered with belts, the equatorial belt, of ochreish colour (inclining to brown), was marked with spots and lines of bright light. There were several broken belts, both north and south; one on the south running in a diagonal direction.